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Widgets: why are some interventions better than others?

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the broader context of service provision, it is important that this does not race ahead of clear prescribing policies, underpinned by pharmacoeconomic considerations.

We agree with the respondents that since buprenorphine is more expensive than methadone, the pharmacoeconomic aspects of prescribing need rigorous investigation. Whilst the studies they cite provide preliminary evidence of equipoise between buprenorphine and methadone, as they point out, these do not adequately take into account the broader effects on quality of life and user satisfaction, as well as other important adverse effects such as overdose. Future pharmacoeconomic studies should also address clinical concerns that, in the context of limited budgets, clinicians are forced to walk a tightrope in balancing quality and quantity in terms of service provision. More expensive treatment runs the risk of reducing the number of individuals for whom treatment is available. We completely agree with the authors that these difficult decisions can only be made in the context of well-designed pharmacoeconomic studies which take multiple factors into account.

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WIDGETS: WHY ARE SOME INTERVENTIONS BETTER THAN OTHERS?

With great interest we read the study by Berglund (2005). This is an interesting attempt to search for possibilities to improve the outcome of treatments in addiction problems. However, we think the methodology used in this study is not correct and has led to wrong conclusions.

In this study, the author uses data from a meta-analysis to show that brief interventions with only one session 'display a robust and stable effect, whereas studies with

several sessions generally show higher effect sizes, but the outcomes were strikingly heterogeneous in contrast to the single-session studies' and as the difference between single session interventions (mean effect size $d = 0.19$) and repeated session interventions ($d = 0.61$) was significant, the author considers these findings as evidence that single session interventions are less effective than repeated session interventions. Based on these data he states that the major research question is 'what is happening from the first to the second session?'

We think that it is not correct to conclude from these data that single session interventions are less effective than repeated session interventions for several reasons. First, as the author himself indicates, there were large differences in populations and designs of the studies. The single session studies might for example have been performed in patients less willing to undergo more extended treatment. Second, the length of follow-up was not taken into consideration in the calculations of effect sizes even though one may assume that shorter interventions had shorter lengths of follow-up and therefore had less room for spontaneous improvements. Third, the author did not provide a rationale for dichotomizing the sessions into single and repeated sessions. We do not know what the results would have been if the cut off score had been different. Fourth, visual inspection of the effect sizes displayed in Figure 1 of the paper by Berglund, suggests that it is not the number of sessions which determine the effect sizes, but rather that all interventions are equally effective, except for two outliers (the studies by Fleming *et al.* 1999; Wutzke *et al.* 2002). And finally, the conclusion that single session interventions are less effective than repeated session interventions is not in agreement with other meta-analytic reviews. Recently, a high-quality meta-analysis of brief interventions also examined the difference between single session interventions and repeated session intervention (Ballesteros *et al.* 2004). In fact, seven of the ten studies included in the Berglund review were also included in the meta-analysis from Ballesteros and colleagues. Unfortunately, this meta-analysis did not find a difference between the two types of interventions (OR = 1.62, 95% CI: 1.36–1.92 for single session interventions and OR = 1.48, 95% CI: 1.14–1.92 for repeated session interventions).

In a case like this, one cannot simply take one characteristic of these studies (one versus repeated sessions) and state that this is the element which determines the effect size. The reason why the effect sizes between studies differ so much can be found in many possible variables, such as length of follow-up, contents or other characteristics of the intervention and the setting, and all kinds of characteristics of the population. Choosing one or more of these characteristics as the core variable that determines the effect sizes is a complex enterprise. Usually, meta-regres-

sion techniques are used to examine possible predictors of effect sizes (Smith & Egger 2001). But meta-regression analyses require many more studies than are available in this meta-analysis, so that is not helpful here.

So, unfortunately, the major research question is not 'what is happening from the first to the second session?' The major research question still is: 'Why are some interventions more effective than others?' and the paper by Berglund does not result in a new answer to this question.

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LOOKING FOR A BETTER WIDGET: RESPONSE TO CUIJPERS AND VAN STRATEN

In the letter by Cuijpers and van Straten (2005) they object to the methodology and conclusions in the brief intervention part of my paper 'A better widget? Three lessons for improving addiction treatment from a meta-analytic study' (Berglund 2005). They specifically criticize the conclusions of the study, the cut-off point used (single session versus multiple sessions) and the results because of differences compared with another study. They conclude that the major research question still is 'Why are some interventions more effective than others?'

The aim of the widget article was to use a unique database in an alternative and more integrated way. The Swedish Council on Technology Assessment in Health Care (SBU) database (Berglund, Thelander & Johnsson 2003) consists of a systematic review of randomized controlled studies in brief intervention and treatment of alcohol and drug problems and a review of long-term studies. In most analyses standardized effect sizes were calculated. Conclusions were presented about evidence-based interventions and treatment for alcohol and drug disorders including brief intervention in risky alcohol consumption (Berglund, Thelander & Johnsson 2003, Berglund *et al.* 2003).

In the widget article this database is used to study transitional or critical phases in intervention and treatment of addictive disorders in order to formulate new research questions and approaches based on unique systematic data. Four issues were discussed, what happens between the first and second session in brief intervention, comparison of effect sizes in treatment of alcohol, opiate and cocaine dependence, interaction between psychosocial and medication treatment and relation between short-term and long-term outcome. In trying to formulate new approaches the time axis was used as the base for analysis. (Intervention – short-term treatment – long-term treatment – short-term outcome – long-term outcome.)

The chosen cut-off point, one versus several sessions, is, with this background, a logical choice. A single session includes the components known to be effective in brief intervention (information about alcohol consumption and health risks and strategies to decrease alcohol intake). In the second (and later sessions) other factors related to follow-up procedures influence on the efficacy of that session (for example repeated sessions, other reinforcing activities and monitoring), all with less documented efficacy.

My conclusion was that intervention studies with one single session showed a small but robust homogeneous effect size, whereas studies of intervention with several sessions were heterogeneous with large and small effect sizes among the included studies. In the discussion part I specifically discussed the studies with large effect sizes in order to better understand improvement between first and second session (which could be related to follow-up procedures and not necessary to intensity of intervention).

Cuijpers and van Straten (2005) also stress differences between my results and those of Ballesteros *et al.* (2004). However, Ballesteros *et al.* included in their brief intervention group studies with simple reinforcing visits through follow-up of about 3–5 minutes each and compared them with an extended intervention group. Several of the studies, which were included in my group of several interventions, were included in the brief intervention group of Ballesteros *et al.* explaining some of the differences found.

Cuijpers and van Straten (2005) conclude that the main research question still is 'Why are some interventions more effective than others?' I do not fully agree with this conclusion.

First, efficacy of brief intervention in health care has been generally accepted for more than 10 years and numerous attempts to introduce the method in clinical practice have been performed but with limited success (Beich, Thorsen & Rollnick 2003). This certainly is related to several factors, but the present research agenda as formulated by Cuijpers and van Straten has not facilitated the issue of effective implementation and therefore there is need for alternative strategies.

Second, interventions with similar effect sizes as for brief interventions in health care have been reported using web-based interventions (for example Kypri *et al.* 2004). Possibly such a technique could be cheaper, more useable and more accepted both by patients and physicians than presently used techniques. An anonymous web-based drinking feedback form in the waiting room with optional follow-up procedure by the physician could be a future alternative. The research question in this case is the efficacy of both the intervention and the follow-up procedures, which have similarities with the question formulated in my paper.

Third, recommendation to brief interventions with and without monitoring (follow-up procedures) has been proposed (Room, Babor & Rehm 2005). In such an approach persons who score in the low positive range (8–15) on the Alcohol Use Disorder test (AUDIT) should receive a brief intervention, those who score in the intermediate range (16–19) need a brief intervention and regular monitoring and those in the high range (20–40) diagnostic assessment and treatment. Follow-up procedures therefore could be an integrated part of a general intervention–treatment package for alcohol problems. Since follow-up procedures are initiated in all cases with more than one session the approach suggested in my paper could be a valid one.

To conclude: in order to develop better models for intervention and treatment of addiction disorders, areas with strong heterogeneity of effects could be identified in order to reformulate the general research agenda. Brief intervention in a non-treatment seeking population is such an area.

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ZOLPIDEM: INTRAVENOUS MISUSE IN DRUG ABUSERS

Zolpidem, a short-acting imidazopyridine, has been used for over a decade in more than 100 countries for the short-term treatment of insomnia. Epidemiological studies indicate a remarkably low incidence of zolpidem abuse compared to benzodiazepines used to treat sleep disorders [1]. Cases that do occur typically involve patients with a history of abusing other drugs [1–3]. As far as we know, only two cases of zolpidem intravenous injection have been reported, both involving drug addicts [4, 5]. In a re-examination of our own data on psychoactive drug consumption [6], we found only seven reports of zolpidem injection amongst 460 patients who visited a drug addiction center in northern France. The number of zolpidem injectors could be underestimated, however, simply because clinicians do not explicitly question drug addicts about it. We therefore carried out a survey to determine the prevalence and geographical extent of zolpidem intravenous misuse in France.

Specific questionnaires about drug and zolpidem injection were given to patients who visited one of five drug addiction centres throughout France over a 3-month period. Surprisingly, only three new cases of zolpidem injection were identified out of 733 patients interviewed. The lifetime prevalence of intravenous misuse was thus 0.41% [0.09–1.19] 95% CI in our sample and

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